for his future lectures. Thus the specimens may be said to *live* in his pages, with all their bright motley of colour and their extraordinary odours—only *flattened* a little by the supreme necessities of the case.

Mingled with the paradoxes, and generally more or less directly suggested by them, we have many valuable pieces of information—as, for instance, about the calendar (pp. 219, &c.), the names of the "beast" (p. 403), the "Macclesfield Letters" (p. 448), &c.—and we have anecdotes, verses more or less confessedly doggrel, and paradoxes full-blown, from the author's own pen.

One extract must suffice, though there are hundreds equally good, for which we must refer the reader to this most thoroughly enjoyable book itself. Our choice is determined by the present aspect of the education question: and conveys a much-needed lesson to all who are capable of comprehending.

"It was somewhat more than twenty years after I had thus heard a Cambridge tutor show sense of the true place of Horner's method, that a pupil of mine who had passed on to Cambridge was desired by his College tutor to solve a certain cubic equation—one of an integer root of two figures. In a minute the work and answer were presented by Horner's method. 'How!' said the tutor, 'this can't be, you know.' 'There is the answer, sir,' said my pupil, greatly amused, for my pupils learnt not only Horner's method, but the estimation it held at Cambridge. 'Yes,' said the tutor, 'there is the answer, certainly; but it *stands to reason* that a cubic equation cannot be solved in this space.' He then sat down, went through a process about ten times as long, and then said with triumph, 'There! that is the way to solve a cubic equation!' I think the tutor in this case was never matched, except by the country organist. A master of the instrument went into the organ-loft during service, and asked the organist to let him *play the congregation out*; consent was given. The stranger, when the time came, began a voluntary, which made the people open their ears, and wonder who had got into the loft; they kept their places to enjoy the treat. When the organist saw this, he pushed the interloper off the stool, with 'You'll never play 'em out this side Christmas.' He then began his own drone, and the congregation began to move quietly away. 'There!' said he, 'that's the way to play 'em out!'"

BURMEISTER'S ANNALS OF THE PUBLIC MUSEUM OF BUENOS AYRES

Anales del Museo Publico de Buenos Ayres, para dar a conocer los objetos de Historia Natural nuevos o poco conocidos conservados en este establecimento. Por German Burmeister, M.D. Vol. ii., parts 1—4. (Buenos Ayres and London: Taylor and Francis.)

IN a previous number of NATURE (vol. iii. p. 282), Prof. Flower has given our readers an account of the first volume of this most meritorious work, and of the objects of its distinguished author in undertaking it. Since Prof. Flowers's article was published, four parts of the second volume of the "Anales" have been issued, containing a series of articles and illustrations of quite as great zoological interest as those in the first volume. The wonders of the extinct Mammalian Fauna of the Argentine Republic are well known, and in the present volume Prof. Burmeister devotes himself to their exposition. In the first part he commences a complete monograph of the Glyptodonts, or extinct gigantic Armadillos, represented by specimens in the museum under his charges and carries it on to the end of Part IV. In the first volume of the "Annals" Prof. Burmeister, in the course of a general article on the fossil mammals of the diluvium of Buenos Ayres, had given a preliminary exposition of his views as to the arrangement of these wonderful animals. He now enters at length upon the description of the species known to him, and gives a series of splendid lithographs to illustrate their remains. Not only are the bones of the Glyptodonts so perfectly preserved as to enable many of the skeletons to be completely restored, but great portions of the extraordinary suits of armour with which they were clad above and below have likewise been discovered, so that their external appearance can likewise be portrayed. Those who interest themselves in palæontology will do well to secure copies of these beautiful illustrations, a few of which are on sale at Messrs. Taylor and Francis, of Red Lion Court, at 10*s.* a number.

We should add that, attached to each number of the "Anales" is a "Boletin del Museo Publico de Buenos Ayres," in which is given an account of the additions made to the establishment during the year. An important acquisition in 1871 was the series of remains of the *Machrauchenia patachonica*, an extinct animal allied to the horses and tapirs, formerly belonging to a naturalist named Bravard, who was killed in the earthquake of Mendoza. These specimens formed the basis of Prof. Burmeister's complete restoration of this animal, published in the first volume of the "Anales."

OUR BOOK SHELF

Notes on the Post-pliocene Geology of Canada, &c. By J. W. Dawson, LL.D., F.R.S., F.G.S. (Montreal: Mitchell and Wilson, 1872.)

THESE "Notes," which are reprinted from the *Canadian Naturalist*, cannot fail to interest European glacialists. Especially valuable for purposes of comparison are the detailed notes on the fossils obtained from the glacial beds. The lists include in all about 205 species, distributed as follows:—Radiata, 24; Mollusca, 140; Articulata, 26; Vertebrata, 5. All these, with three or four exceptions, may be affirmed, says the author, to be living northern or southern species. Moreover, the fauna of the older part of the Canadian glacial deposits is more Arctic in character than that of the modern part. It would thus appear that since the accumulation of the boulder-clay a gradual amelioration of climate has taken place; but the change from Arctic conditions has evidently been less decided on the west than on the east side of the Atlantic. Dr. Dawson's conclusions regarding what we may term the physics of the glacial epoch will probably meet with less acceptance than his palæontological results. He considers the Erie-clay described by Whittlesey, Newberry, and others to be of marine, and not of fresh-water origin, as these authors believe. But his reasons for this opinion can hardly be considered satisfactory. When an extensive deposit of fine clay, after having been examined over a wide area, is found not only to be totally destitute of marine organisms, but to contain quantities of drift-wood, and to have associated with it beds of peat and an old soil containing tree roots, the probabilities are that the clay-beds are of fresh-water origin. Besides, if we are not mistaken, fresh-water shells have been got in the Erie clay. That much-vexed question, the origin of boulder-clay comes in for some discussion in these "Notes," the author inclining to think the deposit is marine. It is somewhat significant, however, that the boulder-clay is only

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fossiliferous in the lower part of the St. Lawrence river; further inland it has not been observed to contain fossils. From the author's description of the boulder-clay as seen at low levels in Canada, we think that deposit more closely resembles some of the maritime fossiliferous stony clays of Britain than our Till or lower boulder-clay. Dr. Dawson seems to have satisfied himself that the "real cause" of the excavation of the American lakes "was obviously the flowing of cold currents over the American land during its submergence." He also thinks that "the fiords on coasts, like the deep lateral valleys of mountains, are evidences of the action of waves, rather than that of ice." No glacialist, as far as we know, holds the extravagant belief that fiords have been cut out by ice. They are undoubtedly submerged valleys, and were hollowed out by streams and other atmospheric influences in ages long anterior to the glacial epoch. But however much we may differ from Dr. Dawson in some of his conclusions, there can be no doubt that he has added very considerably to our knowledge of American glacial deposits, and we cordially recommend the perusal of his "Notes" to our geological readers.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Invention of the Water-Air-Pump

STATEMENT BY PROF. BUNSEN*

"A letter addressed to me by Dr. Sprengel, under date of November 1, 1872, in which he says: 'Perhaps it will not have escaped your observation, that the invention of the water-air-pump, which you have constructed after the principle of my mercury air-pump, according to your paper published in 1868 on the washing of precipitates, is almost everywhere attributed to you,' induces me to make the following statement:

"The interesting discovery, that by means of columns of liquids flowing downwards a more perfect vacuum can be produced, than was possible by the air-pumps hitherto in use, belongs solely and only to Dr. Sprengel. He in his researches on the vacuum (*Journal of the Chemical Society*, January 1865) brings prominently forward, that water is from a practical point of view the only liquid which could come into consideration as a substitute for mercury used in the instrument described by him; and that it is not unlikely that such an instrument, adapted for water, might possess advantages which air-pumps of other constructions have not, particularly in hilly countries, where the large volume of a natural waterfall might be rendered available. In the theoretical considerations on the action of his instrument, which immediately follows the above, it is noticed, that it is simply the reverse of the Trompe, with this addition, that the supply of air is limited, while that in the Trompe is unlimited.

"If in the face of these facts, which are open to all, anyone attributes to me, as I must conclude from Dr. Sprengel's letter, a share in his discovery, I can regret this only all the more keenly, as in my treatise on the new method of filtration I could not possibly have expressed myself with regard to Dr. Sprengel's claims more loyally and precisely than I have done. There, I have stated expressly, that I have constructed the pump used for filtrations and described by me in detail, after the principle of Sprengel's mercury-air-pump. It was the only apparatus of the kind which Dr. Sprengel described, consequently the one to which alone I could refer. "R. BUNSEN

"Heidelberg, Nov. 5, 1872"

Expressing my best thanks to Prof. Bunsen for the above statement, I beg to add, that since 1860 I have been using for laboratory purposes a water-trompe, as described by me in *Poggendorff's Annalen* for 1861, vol. cxii., which (by reversing the action) led me in 1863 to the new method of air-rarefaction. Water was the first liquid, which I used in my first pump, constructed during the summer of 1863. But the fallacies arising from the tension of aqueous vapour and from the air absorbed in water, as well as the inconvenience of having to provide for the requisite fall, caused me to discontinue the use of water, and to substitute in its stead mercury as the most suitable liquid for establishing *the* truth, which I had recognised by means of a water-air-pump with an insufficient fall. My paper of 1865 was written with reference to *all* liquids; in fact, on p. 15 (rendered prominent by italics) I summed up thus:—

"The main fact which I have established in this paper may be shortly stated to be that, *if a liquid be allowed to run down a tube, to the upper part of which a receiver is attached by means of a lateral tube, and if the height at which the receiver is attached be not less than that of the column of the liquid which can be supported by the atmospheric pressure, a vacuum will be formed in the receiver* minus *the tension of the liquid employed.*"

I regret that the obviousness of the matter led me to refrain from expressing myself in a more detailed manner, believing, as I still believe, that what I wrote sufficiently described the construction of the water-air-pump.

In conclusion Mr. Johnson's aspirator* for establishing a current of air ought to be mentioned here. It was recognised by Prof. Hofmann† to act on the principle of the trompe, and of course might have served as an air-pump, had it been noticed at the time that the instrument would furnish the means of creating a vacuum. And I may also draw attention to the tube‡ of a vacuum-pan, through which the water is made to escape, which has served to condense the steam of the boiling liquid. This no doubt would in like manner have served as a complete water-air-pump, but it does not appear that its use as such was discovered. H. SPRENGEL

London, Jan. 22, 1873

Kant on the Retarded Rotation of Planets and Satellites

IT is now recognised that the tides are necessarily lengthening the day; but the history of this recognition seems to be incomplete. "It appears," says Mr. Tait in his "Thermodynamics," p. 86, "that the first suggestion of such an effect is due to Kant." Mr. Stewart speaks more positively ("On Heat," p. 356), but adds that Mayer "was the first to give his conclusions general publicity."

The following are the facts with respect to Kant, as they are to be found in Rosenkranz and Schubert's edition of his works, part vi. pp. vii. 3-12. The Berlin Academy of Sciences had proposed, as the subject of a prize essay for 1754, the questions whether the length of day and night had changed, and if so, what the cause could be and how this was to be ascertained. Kant did not compete; apparently he was dissatisfied, as well he might be, with his attempt to estimate the possible amount of retardation; but he published his views in a Königsberg weekly paper.

It is of some interest to compare Kant's position with our own. In the first place, he expresses himself with almost entire generality. He does not speak merely of the tides, but says that the rotation of any planet is necessarily retarded if it contains a considerable amount of fluid. Kant knew as well as we do that the *considerableness* (that is the magnitude) of the cause affected only the magnitude (and not the bare reality) of the effect; so there is nothing to be added to his statement of the condition of retardation but what our own writers do not seem to think worth adding, namely the energy dissipated in consequence of the imperfect rigidity and elasticity of the solid parts of the planet.

Again, with respect to the final result, Kant makes two statements, which, if literally contradictory, yet taken together go to show the fulness of his knowledge. First he says the rotation must ultimately cease; further on that it must diminish till it is equal to the revolution of the moon, so that the earth will constantly face the moon, as the moon now constantly faces the earth. The essay bears marks of hasty writing; and it seems clear that the latter statement is only intended for that part of the effect which is due to the moon. The former may be intended to affirm the ultimate abolition of the solar day; if it means much more (as it ought) I presume it is inconsistent with Kant's express rejection of the hypothesis of an interplanetary resisting medium.

On the other hand, Kant betrays no suspicion of the reaction upon the disturbing bodies, and the consequent lengthening of the month and year. And in speculating on the possibility of

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* Translated from *Ann. Chem. Pharm.* vol. clxv. p. 159, by H. Sprengel, authorised by Prof. Bunsen.

* *Quarterly Journal of the Chemical Society*, vol. iv. p. 186. 1852.

† Ibid.

‡ "Elements of Physics," by Neil Arnott, M.D. (Longmans.) 3rd edit, London, 1828.

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